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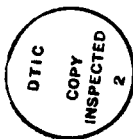
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The Professional Apprentice: Observations on Fieldwork
Roles in Two Organizational Settings¹

John Van Maanen

Deborah Kolb

June, 1982
TR-ONR-5

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Ethnography is the method of choice for organizational researchers who regard the social world as knowable only in terms of the meanings people grasp, create, maintain, transmit, and alter in the process of dealing with one another.² It is a method which seeks order amid the complicated and often strategic human attempts to align action, symbol, and context. Ethnographic research involves participant-observation and fieldwork is the code term for this most unsystematic activity. The rudiments of fieldwork are captured in the following two statements:

(The) analysis that follows was based upon the observation of novice policing in situ. The study was conducted in Union City (a pseudonym) over a nine-month period. Approximately three months of this time were spent as a fully participating member of one Union City Police Academy recruit class. Following the formal training phase of the initiation process, my fully participating role was modified. As a civilian, I spent four months (roughly eight to ten hours a day, six days a week) riding in patrol units operated by a recruit and his FTO (Field Training Officer charged with imputing "street sense" into the neophyte) as a back-seat observer. From the outset, my role as a researcher-qua-researcher was made explicit. To masquerade as a regular police recruit would not only have been problematic but would have raised a number of ethical questions as well... The conversational data (are) drawn primarily from naturally occurring encounters with persons in the police domain (e.g., recruits, veterans, administrators, wives, friends, reporters, court officials, etc.). While formal interviews were conducted with some, the bulk of the data contained here arose from far less structured situations.

(Van Maanen, 1973:408)

Over a two year period, I was a participant observer in these two services (a field office of the Federal Mediation and Conciliation Service and a State Agency of Conciliation and Arbitration). I interviewed mediators, sat around their offices, had lunch and dinner with them and accompanied nine of them on 16 extended cases as an observer. Although the mediators and

parties knew of my research purposes, we agreed that I could best learn the practice of mediation if I were treated as a trainee. Hence, I was, through on-going consultation, taught what they might teach such an apprentice. I reviewed the records with them, learned what they knew about the case prior to its start, was present at all meetings and caucuses including many of the backroom, off-the-record meetings (where deals were reputedly struck). During lulls, the mediator and I discussed his on-going analyses and calculations, his "reading" of the issues, positions, and people, his plans for the "next move," and his accounts of the one just past. Informal interviews with other mediators and spokesmen, attendance at training conferences and perusal of published first-hand accounts supplement the primary case and interview data.

(Kolb, 1981a:3-4)

Descriptions like these, however typical they may be in research journals, are quite crude. Certainly they mask more than they reveal. They read rather like invidious displays of certain conventionalized research practices--length of time spent in the field, brief examples of some of the researcher's activities, sly indications of just how deeply the fieldworker penetrated the studied scene, illustrations of typical roles played in the field, and so forth. One learns very little about field work from such statements.

This essay is an effort to lift the curtain on some practical ethnographic activities by making explicit several individually-specific contrasts in fieldwork practice. The comparison goes beyond that of pointing to our salient biographical and personal characteristics. Although undoubtedly significant, these spritely character contrasts are meaningful only in light of the circumstantial particulars of the organizational and occupational worlds we entered. Our intent is to illustrate first how some ethnographic research came to be accomplished in two distinct social worlds; to note second the role differences that emerged as a result of our everyday research activities; and to suggest third the commonalities that seemed to appear in our methods despite our particular and, no doubt, stylized approaches.

Obviously, with hindsight, sweet reason can be provided to frame much of our action in the field. In this sense, the paper can be read as a nicely worked up purification attempt or as an elaborate accounting exercise designed to clean up our respective research ledgers -- what energies were spent for what purposes with what keen results. As such, readers will do well to keep eyebrows raised and skeptical attitudes toward these materials for, as memoirs and autobiographies not-so-subtly suggest, self-justification (and self-parody) lurk just beneath the surface in undertakings of this type. As Garfinkel (1967:34) and other have almost too cleverly pointed out, when we are called upon to scrutinize some of our activities, we tend to merge the question of what we are doing with the question of what we should be doing. Such tendencies are perhaps even more pronounced when reaching backward for materials to examine.

With this qualifier in mind, we hasten to add that even though there are (inescapably) self-preserving overtones in our method descriptions, additional purposes are served by this essay. First, ethnography is currently something of the parvenue method in organization studies and, although it is frequently discussed and honored as a useful technique, it is not frequently done. Hence, the activities which constitute the method are known by many in only vague and remote ways. By making our contribution to a growing library of first-hand accounts of organizational fieldwork, it may be possible to begin seeing just what ethnographic standards can be said to exist -- be they seen as proper or improper. Second, this essay serves also to highlight some of the background understandings we made use of when going about our work. Thus, the analysis represents another step toward explicating some of the commonsense assumptions which function as the ground upon which our respective research figures (police officers and labor mediators) stand. While it is probably true that we are not the most appropriate ones to be writing the story of our own research procedures, no one else was around with the rather arcane and certainly

peculiar interest to do so. We direct this essay to those with such a special interest.

On Ethnographic Aims

It is quite possible to receive a Ph.D. in a field presumably concerned with human behavior in and of organizations without ever having watched social life in these objects of our affection in any great detail or for any length of time. True, one will have a history more or less rich in organizational membership (family, work organizations, schools, clubs, athletic teams, musical groups, etc.). But, typically, this history is taken as irrelevant to the pursuit of higher learning in the field and exists largely as a residue of unexamined experience (though, perhaps, strong feeling) in which few, if any, analytic percepts are to be found. At best, one's lived-in organizational experiences are treated as a source of example useful only for such mundane matters as teaching and conversation. While personal example is a highly regarded, practically sacred, pedagogic device, such an example is not to be treated as anything more than expressive anecdote, designed to titillate, amuse, arouse, shock, or otherwise gain the attention of what may be a sleepy audience.³

We regard this state of affairs as regrettable. The dismissal of first-hand experience and natural observation as important sources of data and concepts implies a rejection of everyday life as a domain worthy of examination. By everyday life we mean quite literally all the routine, episodic minutia that occupies most of our waking hours (and those whom we study). Here is where we believe crucial organizational data is to be located. Method textbooks are relatively good at telling us how to represent various kinds of data, how to put such data into category schemes, how to assign numbers to the categories, how to manipulate such numbers, and, with increasing sophistication, how to

inspect these numbers for a numbing variety of statistical regularities. But, what it is that can properly be called data must first be snatched from the on-going stream of everyday life whether it is taking place behind a computer console in a university or behind an executive's desk in a corporation.

It is in this domain that ethnographers have begun to carve out a niche among organizational researchers.⁴ Ordinary social science techniques which make much use of surveys, interviews, experiments, and archival records are of little use here since, in the main, such techniques operate (often with more than a little sympathetic magic) to tell us if, when, where, or why something is happening in the world. As to what it is that is happening, these techniques are silent. Their use presumes a definition of the situation, a definition which is, in most cases, the invention of the analyst. Ethnographic research seeks to describe what it is that can be said to be happening from the point of view of those for whom it is happening. This is an awkward way of making an equally awkward point: Ethnographic research is ontological, concerned with the "logical priors" of understanding human events. It seeks, given an acknowledged host of practical and philosophical troubles, to discover what is happening in a given place and time and to pass this discovery on to readers who are presumably unaware of such matters. The mandate is made particularly difficult (and perhaps pretentious as well) because it is not the ethnographer's understanding of what is going on that is sought but rather the understanding of those that populate the studied places and times. In this sense, ethnographers are engaged in a data manufacturing enterprise. In the words of Schwartz and Jacobs (1979:2), ethnographers are in the "reality reconstruction business."

Given this altogether sweeping aim, ethnographers must be concerned with pattern and form if for no other reason than parsimony. Whatever other constraints they face, the paramount one deals with the impossibility of ever achieving

literal and complete description. Here, several widely held ethnographic assumptions prove useful.

First, explanations of organizational behavior (at all levels of analysis) rest fundamentally on the brute, mundane, regularized interaction patterns associated with the day-to-day activities of members of the studied organization. Like the carving out of a shoreline by the incessant slapping of the sea, ethnographers assume everyday life in an organization has more to do with shaping the behavior of the membership and the nature of the organization itself than the prescribed table of organization, the so-called external environment, or the dramatic but apparently rare occasions of high-level decision making. This is simply to say that to understand an organization and its membership, one must know in concrete detail what is typically done by those in the organization.

Second, everyday life within an organization must be treated as analytically interesting in and of itself. A description of how an activity takes place is, to a fieldworker, a finding of substance and theoretical interest. It indicates, among other things, what people are more or less skilled in doing. It is worth noting in this regard that ethnography offers a potential check on what we see as an overconceptualization of the workplace in much of what passes for organization theory. Although people go to work and "make cars" or "teach" or "do marketing surveys;" these activities are commonly seen as mere instances of "programmed (or autonomous) decision making" or "authority displays" or "uncertainty reduction." Whatever skills organizational members may have worked long and hard to develop are as casually dismissed by the analyst as are the members' own definitions for what it is they are doing. For many organizational theorists, the workplace is but a staging ground upon which endless investigations of, say, mental health or social structure can be launched under the full moon. This hardly differs from the

often made comment that people portrayed in the movies or on television are rarely shown working. What to us are places where we spend inordinate amounts of time doing some very practical things, are to our image makers simply background sets against which marriages fall apart, crimes get hatched, personal needs attended to, or evil conspiracies exposed.

Third, to achieve even a tentative grasp upon everyday life in organizational worlds, ethnographers believe they must negotiate a thorny maze which separates outsiders from insiders. These are of course folk categories which can and do take on very different meaning depending on the setting in which a study is conducted. Participant observation inevitably involves hanging around, learning a language, living-in. As a result, fieldworkers hold rather dear the assumption that they must "pay their dues" and "earn" insider status. More to the point, however, fieldworkers are convinced that once granted insider status, special information unavailable to outsiders will be forthcoming. Whether or not such information is, in fact, forthcoming or is, in fact, understood in the same way as others in the scene is, in fact, another matter entirely.

In the remainder of this essay, we concentrate upon some of the nuances involved in our gaining (and occasionally losing) insider status within the two organizational contexts of our studies. What we think emerges from this depiction is, despite a number of surface contrasts, a somewhat striking commonality in our approach to fieldwork, an approach conveyed by the phrase, the professional apprentice. What follows is a set of rather pragmatic fieldwork problems arranged in a loose, sequential fashion (from entry to exit). Each are discussed in terms of how we individually addressed them in our respective research situations. The illustrative materials come from both previously published work of ours or from our unpublished fieldnotes and are so noted in the text. Some stage-setting matters occupy our attention first.

Context

It is neatly the case that persons under the eye of an avowed researcher may very well act in ways knowledgeable of this fact. This human version of the Heisenberg principle has been documented time and time again making any statement attesting to its presence something of a methodological cliché.⁵ What is often overlooked however is the implicit reciprocity embedded in the cliché. That is, while the researcher attends to the study of other persons and their activities, these other persons attend to the study of the researcher and his activities. An underlying theme to the cautionary tales we tell here is that the success of any fieldwork endeavor depends inherently upon the results of the unofficial study the observed undertake of the observer.

At the outset of any ethnographic investigation, the fieldworker represents an audience to the actors being studied. Basic to this actor-audience relationship is the artful striving on the part of the actors to manage a favorable impression of themselves and their work (Goffman, 1959; Berreman, 1962). The police, for example, foster a version of reality that emphasizes among other things their effectiveness as crook catchers, their efficiency as a public bureaucracy, their careful observation of the legal constraints placed upon their mission, the dangers associated with their tasks, and so forth. Similarly, labor mediators characterize themselves as altogether trustworthy, knowledgeable, and impeccably neutral, devoted only to the maintenance of industrial peace, a task they see as undervalued by society. Persons who wish to look behind these impressions to the "backstage" areas where such impressions may be less tightly monitored are discouraged from doing so.⁶ To enter this region, fieldworkers attempt to develop close ties with at least some members (ie, informants) of the studied organization. To establish such relations means that the researcher must, at a minimum, be able to recognize and respect the ritual constraints governing interaction in the specific field of study.⁷

Creating a relationship between the observer and the observed that moves beyond the simple dramaturgic dynamics of impersonal actors and categorical member(s) of an audience is then a fundamental requisite for doing ethnography. The notion hold true whether the people of study are corporate executives, nude sunbathers, or Trobriand Islanders. But, it is also true that the technical problems associated with sharpening up one's character and forming relationships marked by acceptance and perhaps trust reflect various particularities of the social group under study and the demographic and social characteristics of the fieldworker. Research settings differ along many lines -- size, structure, tasks -- and these always specific attributes both constrain and enhance the kinds of relationship to be formed within them. Moreover, the fieldworker is not biographically transparent. Thus, what Goffman (1963:55) regards as our "virtual identities" will also shape in unique ways the kinds of relationships available to the fieldworker in the research setting.

There is, of course, a substantial body of sociological and anthropological writing devoted to fieldwork in general which identifies generic areas of problematic concern.⁸ Certainly these field guides are helpful when planning, organizing, and attempting to focus a study before entering the research setting. But, these prescriptive guidelines are of questionable assistance when one enters a specific social world except, perhaps, to remind one that the difficulties and anxieties raised by involvement in unfamiliar surroundings are common to all strangers, sojourners, tourists, immigrants, recruits, novices, missionaries, and fieldworkers. More to the point, given ^{that} the major purpose of most field studies is to describe the everyday understandings necessary to operate within the bracketed social world under investigation, it is apparent that a thoroughly useful guide for conduct cannot be provided prior to undertaking a study in an alien setting. To know precisely how to study a given community or organization would be to know the community or organization intimately. If it were possible to draw

such a behavioral flow chart, there would be no need to do the ethnographic study.

Two Settings, Two Fieldworkers

To name the occupations of police officer and labor mediator in the same sentence is to strike a small note of discord. On most exogenous dimensions -- social visibility, number of members, location of the work -- these occupations could not appear more different. So too, the researchers. Were readers to see us, the contrasts in our appearance might evoke some amusement. That we were both doctoral students when our fieldwork commenced is perhaps the only obvious point of similarity. Consider some of the differences in our respective research settings and fieldwork identities.

Mediation: The resolution of two-party disputes through the use of a third party who is presumed to be neutral is an ancient method. A prominent, institutionalized application of this process occurs in American labor relations. When union and management are unable to agree on the terms and conditions of their contractual agreements, they are often legally compelled to engage the services of a mediator. Although mediators may come from the ranks of arbitrators, members of academe, or representatives of the clergy, the bulk of labor mediation in the United States is conducted by employees of the Federal Mediation and Conciliation Service (FMCS) or their counterparts in state mediation and conciliation agencies.⁹ A field office of the FMCS employing six mediators and a state agency employing nine mediators were the sites of Kolb's study. Several characteristics of these mediation domains warrant mention here for they bear directly on the kinds of research roles available in such settings.

1. Mediators work by themselves.¹⁰ On any given dispute, they are typically the sole third party present. Each party to a dispute is represented in mediation by a bargaining committee that varies in size from, usually, two to seven members. In turn, each committee will normally have a spokesperson with the responsibility, if not the authority, to speak for the committee as a whole.

2. Agency control over the mediator's activities is accomplished primarily by official record keeping. Mediators in the FMCS and the state agency work within loosely structured, largely decentralized organizations and their behavior is monitored only indirectly. The FCMS field office is part of a regional office located in another city. There is no one mediator at the field office with official responsibilities for the work of the other mediators. Direction from the regional office is via dispute assignments and status reports. In the state agency, a director oversees the arbitration arm of the agency and this task occupies the greater portion of his time. Assignments to a dispute are made by the most recently hired mediator and no status reports are required. The director of the state agency is a political appointment. Mediators typically have longer tenure than the director.
3. The mediators work is case work. Disputes come to mediation via petitions from the parties. Each dispute becomes a case on which the assigned mediator works until it is resolved (or, in the state agency, until the dispute is referred to factfinding). This may take a matter of hours, days, or weeks. With some seasonal variation, mediators handle, on average, 400 cases annually.
4. From the mediators standpoint, each case is different. Since current practice in collective bargaining is to negotiate multiyear labor agreements, the same parties come to mediator only once every two or three years. Thus, the issues in dispute are always seen as variants (large or small) on those issues addressed in previous agreements.

Policing: The organization of police work in the United States is decentralized, each community having its own agency. The police task is defined reactively, largely in terms of a department's response to citizen requests for a bewildering array of services. Such demand is seen as more or less spontaneous (Manning, 1981) and is handled, by and large, by members of the patrol division, the largest division within any U.S. police organization. Van Maanen's study took place primarily in the training and patrol divisions of a large, urban police agency employing roughly 1200 uniformed police officers. Other occupational particulars bear mention.

1. Patrol work is teamwork. Geographical districts of the city represent beats to which squads of five to 25 officers are assigned under the occasionally watchful eye of a sergeant. Each squad works one of three shifts such that all beats are covered (theoretically) on a 24-hour basis.

2. Recruitment into the occupation is typically associated with kin and friendship networks and is overseen by the local Civil Service Bureau. With some departmental variation, policing is very close to an all-male occupation. Training in large police departments includes both formal instruction in a Police Academy and informal tutelage on the street.
3. Direct communication from the dispatch center of police organizations to officers on the beat bypasses the sergeant. Such communications are formally defined as "orders" in the chain of command, thus, the paramilitary structure of the police -- within which sergeants stand as direct supervisors to the men assigned them -- is best considered a bureaucracy in form only. Much of what squad members do, they do out of sight of their sergeant (and one another) and do so, not at his command, but at the request of radio.
4. There is no lateral entry in police organization except at the very top of the hierarchical structure. Virtually all members enter at the ground level and build their careers outward (and, for some, upward) from the patrol division. In a very real sense, routine patrol activities are considered by members to be the "essense" of police work.

The Fieldworker, Deborah Kolb: The mediation study was initiated by Kolb while she was a doctoral student at MIT and was directed toward mediation occurring in and around the Boston area. Only modest amounts of travel were required. In this regard, several biographical specifics are relevant.

1. Appearance: Kolb was 34 at the time her study began. She is a short, Jewish mother-of-two whose appearance contrasted sharply with the predominantly Italian and Irish male mediators working in the agencies she studied. The mediators were mainly in their 40s and 50s, married, with active family lives overseen by a wife who did not work outside the home. Only two of the mediators were proximate in age to Kolb and only one mediator was female.
2. Involvement: Residing in her home community with acknowledged family responsibilities, Kolb's relationship to the field was defined primarily as that of "work." Like the mediators, work was depicted as something done in the office and not to be taken home. For the most part, relationships with mediators were restricted to the spatial and temporal boundaries of their working day. The line between work and leisure was clearly demarcated by both the fieldworker and the mediators.
3. Status: As a graduate student in a well-known institution containing several faculty members equally well-known in the so-called industrial relations community of which labor mediators are a part, Kolb's identity as a well-placed and perhaps influential student was apparent to most mediators encountered in the field.

Given Kolb's demographic particulars, her limited involvement in the lives of the mediators studied, and the salience of her institutional affiliation, the fieldwork role she tried to develop was modelled after that most appreciative student, the "trainee." In contrast, Van Maanen's position was modelled more along the lines of a member or "colleague" in the organization, albeit a rookie. This fieldwork role came about by sharing experiences with a cohort group of recruits and was aided by his appearance and personal circumstances at the time of the study.

1. Appearance: When initiating his police studies, Van Maanen was 26 years old, younger than several recruits in his Academy cohort and only a year or so older than the average age of his class (about 25). He is a tall, caucasian male from a middle class background that more or less mirrored the backgrounds of most police recruits -- about 30 percent of whom were new to the city and region. With the aid of barber shears and minor alterations in attire, Van Maanen was easily able to at least look the part of a police recruit.
2. Involvement: Van Maanen, along with his wife Colleen, Union City, a sprawling metropolis considerable distance from the University of California, Irvine (UCI) where he was then a graduate student. Both he and his wife, geographically separated from their social, familial, and previous occupational ties, participated rather fully in the everyday life of the rather tightly linked police community. Police recruits and their families became anchors in a sea of unfamiliar faces; not merely as subjects or informants, but as friends with whom to share a part of one's life.
3. Status: Van Maanen sought actively to downplay whatever status his university affiliation might bring. This was, to a degree, eased because at the time (and probably now) UCI was hardly an institution of widespread fame. Nor were any of Van Maanen's faculty advisors familiar to the recruits. The symbolic connotations associated with the tag 'Ph.D. student' were, however, many, although, no doubt, of mixed significance and meaning to the police. Two other recruits (in a beginning class of 44) were also working toward advanced degrees (both Master's degrees in a local university's public administration program). While Van Maanen's anomalous status was undeniably salient, the influence it had on relationships carved out in the rigidly prescribed activity schedule of the police academy was, in all likelihood, less crucial than his day-to-day conduct as a member of the class. Whatever credentials were earned as an academy class member -- as recruit, rookie, colleague, good guy, asshole, or pencil-necked geek -- were carried over to the patrol division where Van Maanen worked under the official canopy of "reserve officer."

These contrasts in appearance, involvement and status provide the borders within which we framed our fieldwork. Kolb's borders were relatively less ambiguous than Van Maanen's and, as such, were less subject to manipulation. The intermittent character of mediation work coupled with the fact that only mediator's mediate meant that Kolb's research role would be built primarily upon the options open to an observer of the scene. Yet, these options were multiple and Kolb, using various aspects of her identification as a graduate student of labor relations at MIT, eventually took on a role structurally similar to trainees in the occupation. For Van Maanen, who could move more readily into the demographic landscape of the studied setting, the hierarchical character of the organization, the group-based work practices found there, and the altogether intermeshed nature of Van Maanen's work and leisure activities while in Union City gave rise to a collegial relationship with police officers. These two fieldwork orientations or stances were, to be sure, not achieved by any prestudy fiat or design. Although the roles of trainee and colleague were dimly perceived by each of us before going to the field, it was a most idealized version that was held in mind. Both roles had to be carved out individually within each setting (and, carved out time and time again). It is to this role making process we now turn.

Primary Access: Getting Past the Gatekeepers

Most fieldworkers would probably agree that gaining access to most organizational settings is not a matter to be taken lightly but one that involves some combination of strategic planning, hard work, and dumb luck. When reading the polished results of ethnographic study, facile treatments of the fieldworker's entry into the setting are often given. It is as if the fieldworker simply approached the relevant gatekeepers in the organization, patiently explained the purposes of study, and then, presto, access was eagerly granted. Certainly, this sort of Arthur Murray version of the Entry Waltz did not fit our accessing process nor did it resemble the advice we received from our close academic counselors. What we were told and

what we indeed discovered later on our own was the importance of a sponsor to ease our way past the gatekeepers. Such a sponsor would act as a go-between or intermediary who would actively support our research plans to the organizational gatekeepers and be viewed as trustworthy by them and thus accountable to them. In both our studies, it was through the efforts of such go-betweens that access was eventually secured. Consider the ways these intermediaries participated in getting our work off the ground.

Most crucial to the process of securing access was a contact I developed at UCI while in the midst of seeking a representative American police department (ie, large and urban) within which to conduct my work. After six frustrating months of attempting to gain access to a police agency, I discovered, almost by happenstance, a faculty member in the psychology department at Irvine who had once run a series of encounter groups with the upper echelon police officers in Union City. I sought out this professor, told him of my general plans and interests, and asked him for any assistance he might be willing to provide. I also told him of the great difficulties I was having getting into a police agency. At the time we talked, I had been denied access to fourteen departments on various and sundry grounds, the most popular of which seemed to be the legal complications my presence in their particular department would create. At any rate, this faculty member agreed to help and using the rapport that perhaps only a sensitivity trainer can achieve, was able to persuade the command in Union City of the merits of my planned study and approach. The rest of the negotiations followed in a rather hurried, though somewhat pro forma fashion. Within a week, I flew to Union City, met with the Chief of Police and several of his aids. After an afternoon of meetings with these men, I was granted access to the department on what could only be called open terms.

(Van Maanen, 1984:195-6)

The pyramidal structure of the Union City Police Department meant that securing access for the study depended crucially on clearance from the chief's office. Once an agreement had been achieved with the high officials of the agency, Van Maanen became a member of the next academy class and the project officially commenced.

The loose structural link between the administration of the agency and the work of the mediators meant that in Kolb's case, negotiations for entry was a less direct matter -- clearance from the senior FMCS mediator and state agency director and then repeated individual negotiations with different mediators

regarding observation on particular cases. In both phases, the support of two intermediaries, each faculty members at MIT, provided unmeasurable assistance.

My official clearance was accomplished largely through the efforts of two faculty members in the Sloan School. One, the head of the Industrial Relations Section at the time, knew the senior mediator at the FMCS office. My access there was quite smooth. The section director wrote a letter of introduction to the field mediator, vouching for my credentials and scientific worth of the proposed study. My initial phone call was greeted with recognition and welcome. I later learned that some 25 years earlier, this mediator had facilitated another doctoral dissertation advised by the department chairman, then a faculty member in the Economics Department at MIT. My early meetings with this senior mediator were casual and friendly. He explained mediation to me and I described my purpose to him -- "to get the mediator's perspective on their cases." During our first meeting, we set up the procedures for my participation on FMCS cases.

My entry to the state agency was not so smooth. A dean at the Sloan School knew the director of the state agency and had recently worked on a research project with him and mentioned my research interests. When I contacted the director, he expressed concern about all aspects of my study: problems of confidentiality, the potential nuisance my presence on the scene might create for mediators, the possible deleterious effects my presence might have on the parties to the dispute, and so on. On several occasions, the dean called the director to again vouch for me and to indicate his personal interest in the study. The director finally agreed to let me enter with the warning that if he heard any complaints about me from any of the mediators, I was out. He said later his actions could be understood because "he owed the dean a favor."

(Kolb, 1981b: 346-7)

Kolb's negotiations with individual mediators also differed by agency and the nature of the bargains she struck in the field depended upon the personal preferences of the mediators.

Despite gestures of cooperation, actually attending a federal case proved to be more complex and time consuming than I had at first thought. Two months elapsed before I saw my first case. Our arrangements were for me to call each mediator at the start of the week to see what cases he had scheduled. Because of the case monitoring process they use, federal mediators often do not know which of the cases they follow will require direct intervention until right before a meeting is held. Further, I started the study at a sluggish time of year (and, at the beginning of the month when caseloads are typically light). In retrospect, I think they wished to minimize the awkwardness my appearance on a case presented to them. Thus, they waited for a case where they knew the parties (spokepersons) well, either personally or by reputation.

Arranging cases with the state agency proved less time consuming and detailed. The state caseload was more stable; cases started continuously and ran for long periods of time. After I was cleared by the agency director, I called each of the mediators, introduced myself and the purpose of my study, and then asked to accompany them on a case. We often set a date at that time. Occasionally, I had to call back because their calendar was blank. This occurred infrequently in comparison with the federal mediators. Insofar as I know, state mediators did not clear my presence with anyone prior to the start of a case.

(Kolb, 1981b: 319-50)

It is clear from our experience that the process of securing formal clearance is shaped by the structure (and its enactment) of the setting. When official gatekeepers indeed speak for their organization, they may be reluctant to commit the resources under their aegis to an academic study they perceive as tangential (or possibly embarrassing) to the work of their organization. But, once such a commitment is made, the way is cleared for the study to commence almost immediately. Van Maanen began his fieldwork less than two weeks after his chat with the chief and Kolb quickly began case observations with state mediators once having secured the director's reluctant approval (lest he change his mind). In organizations where influence is more dispersed and decentralized, initial clearance may be easier to secure but access to the locations of interest is not guaranteed. It took months for Kolb to arrange for her first FMCS case to observe after clearance had been achieved. Whatever the structure of the setting, however, our experience confirms the adage that the wise researcher is well advised to seek out intermediaries who can counsel, set up contacts, run interference, and otherwise attest to the fieldworker's exemplary motives and character when attempting to break into an organization. Another of Kolb's examples makes this general point well.

Beyond assisting my entry, the dean and the department head continued to facilitate my research role. All the mediators and many of the parties knew these two either personally or through the grapevine. Mediators often used my illustrious connections to introduce me to the parties (partly, I think, to justify to the parties their collaboration in allowing me to be there). When introduced to a bargaining representative, the mediator would often say: "This is Debbie Kolb. She's doing a study on mediators over at MIT with Charlie and Abe." Such an introduction invariably provided an occasion for a

warm greeting and a story about the last case they had worked. Sometimes it provided the cue for a joke to be told at the expense of the absent Charlie or Abe. Awkward moments were filled with conversations of this type, filling the time such that a description of my research given to the parties proved often to be unnecessary. Apparently, insofar as the parties were concerned, if I knew Charley or Abe, I was OK by them.

(Kolb, 1981b: 349)

Initial Bargains: What gatekeepers offer is access to the setting. But what they expect in return for their clearance is not always apparent. Indeed, their demands may be minimal. Consider Van Maanen's experience in this regard.

No editorial control was asked for nor was there any direct discussion of what the police (ie, the chief) desired from this initial research bargain. I did promise, however, not to identify people, places, or, significantly, the department in whatever writing was to follow the study. Also, I mentioned to several police administrators in the department that I would be happy to provide them with written reports on my progress whenever they requested. Surprisingly, none were ever requested and, once the study was underway, I did not repeat my offer. The only apparent concern voiced by the police command was that I pay my own way.

(Van Maanen, 1984: 196)

Those who spoke for the mediators of Kolb's study also seemed relatively unconcerned about what they might receive from the study or what might later appear in print. Rather, they emphasized certain rules of participation that Kolb was expected to follow.

The extent of my research contract with FMCS was threefold: I respect the wishes of the individual mediators; my presence on a case be cleared by the parties; and, certain cases might be closed to me. The state agency director set down more prior constraints and in more specific form. Perhaps as a result, these proved to be quite malleable. He barred me from recording proceedings in any way; he forbid my attendance at any off-the-record meeting; and, he required that he give clearance before any of my findings could be published. These were in addition to others similar to those of the FMCS. Yet, none of these rules were ever enforced. Since the director's activities were devoted primarily to the arbitration side of agency, he was quite distant from the day-to-day mediation concerns. All the agreements about transcription and participation were negotiated with individual mediators, most of whom had worked for the agency before he arrived and expected to be there after he left. It also seemed at times that the state mediators were eager to thwart his authority

which included of course the rules he'd laid down for my conduct. At any rate, beyond the gatekeeping function, the director played no further role in the research and was gone from office before my study was completed.

(Kolb, 1981b: 346-7)

In balance, what the studied organizations offered the fieldworkers was a rather special sort of access to their members -- an access certified by those who claimed authority in the organization. What these authorities requested in return were agreements on procedural matters and some most ambiguous constraints on whatever publications our studies might involve. Left unsaid in these preliminary bargains was apparently an implicit trust placed in us that we would faithfully adhere to our part of the arrangement. That such trust is inevitably violated is a topic we shall return to again.

Secondary Access: Sizing-up the Fieldworker¹²

Passage through the official points of entry permits the observer to enter the field. It is at this point that the observed initiate their study of the observer. It is one thing for the researcher to claim that he or she is acting in a warm, confidential manner and quite another to be seen as acting in such a fashion by the subjects of the research. How the researcher manages such impressions (well or poorly) is the topic treated in this section.

Assessing the fieldworker's commitment to the study (and, to those studied) seems to be an early concern of the observed. If the fieldworker claims to seek an understanding for what it is those studied are up to is to be credited by them, then behavioral indications must be forthcoming from the fieldworker that suggests he is actually putting in the time and effort to do so. Such time and effort must more or less parallel the notions carried by the members as to 'what is necessary.' Van Maanen's

attendance at the police academy and his persistent presence in the patrol division seems to have driven such commitment home.

I entered the police academy as an avowed researcher, who, I wanted made known, would stay with the class through graduation and presumably spend an indeterminate amount of time working with recruits after they had left the academy. To a police researcher seeking to understand the police milieu, attendance and matriculation from the academy serves the expressive function of providing the fieldworker with at least one common and, for policemen, significant experience to share with other members of the organization. As one veteran policeman told me, "anyone willing to put up with that academy bullshit can't be all bad."

(Van Maanen, 1978: 338)

In mediation, there is not an analogous commitment-posturing device to that of the police academy. But the need for the fieldworker to enact or document commitment is equally important. Since mediators believe their work to be something of an art or craft whose skillful performance can be understood only in its unfolding situational expressions, attendance on a case from start to finish helps signal the fieldworker's behavioral commitment to the study. Attendance on many cases provides the clincher.

The first case I observed with the federal mediators ran four, nine-hour days before it was resolved. I showed up at nine and left at six each day. Milling around the office, the other mediators remarked on my diligence -- "you're getting to be a fixture around this place." So too, with state mediators. Driving home at four in the morning following a case that had settled after two long days and nights, the mediator remarked, "I don't know whay you're doing this, at least I get paid."

(Kolb, 1979: 4)

Whereas evidence of commitment may convince the studied that the fieldworker means business and is indeed willing to learn the complex ins-and-outs of organizational activity from, so to speak, the horse's mouth, the fieldworker may still find himself without a mouth from which to learn. Here, impressions of reliability must be developed and more or less sustained. By reliability, we mean merely the degree to which the observed believe the observer can be counted on to act in accordance with certain standards of propriety and certain

standards of accepted occupational practice (be they formal or informal). In this domain, there is a nifty parallel to the substantive aims of study for these very standards within which the fieldworker must act are the elusive targets of his research attention. Steady and focused attention in this sphere comes as close as possible to offering the researcher an operational definition of praxis. Reliability testing, while continuous throughout a study, begins early. Consider one of Van Maanen's comments on this process.

"Fear, to the observer of the police, stems from several sources. Certainly, by associating closely with the police it may come from the ever present danger existing in city streets. But, fear also may arise from the police themselves. The police, of necessity, are not gentle, impassionate sorts who can easily tolerate a deviant in their midst. And, the working style of an observer is sure to reflect this. Of course, one cannot know until the moment arises, how he will handle these fears. But, the police will certainly be watching to determine if they can "depend" on the researcher ...

(Van Maanen, 1981a: 197)

Reliability in the context of mediation touched on the issue of confidentiality and the mediator's continual desire for assurance that it would not be violated in the course of a case.

The mediators in both agencies directly and indirectly expressed concern about the confidentiality of the process. Indeed, certain mediators precluded any transcription for apparently just this reason. Picking up on this clue, I adopted a particular posture in off-the-record meetings, those encounters between mediators and chief negotiators where I felt the issues of confidentiality would be most salient. I stood apart from the participants looking at my feet, careful to not comment nor pay too much eager attention to what was transpiring at the time.

(Kolb, 1977: 4)

Behavioral commitment signals interest and judged reliability suggests how far this interest can be taken. Relationships in the field however vary by person and vary over time. Such relationships involve the observed's notions about the fieldworkers commitment and reliability but go well beyond such matters. Fundamentally, research relationships, like all human ties, are

premised on trust when they are close, distrust when they are not. Simple affection or liking another is not a crucial concern in this regard for one can easily like another without trusting them (and vice-versa). To achieve the bland sobriquet of "good guy" in the field is hardly a matter of great mystery or difficulty -- what else can a fieldworker do but express (tactically or otherwise) a concern for others, a willingness to listen to other's problems and worries, a desire to do little favors for people, and so on. One must not be a cipher of course but being the "good guy" is relatively easy when it is one's main involvement. Trust may sometimes be built on such grounds but trust also cuts considerably deeper into the informational core of research relationships than does mere affection.

In this context, trust is best thought of as a massive background feature of being. As such, it can only be revealed to others through social interaction (Garfinkel, 1963:187-238). It is not something one possesses but rather is something one does. Although trust may be signalled and received in many ways, we will treat only the identification and allegiance aspects of trust here.

The choice of identification with a particular organizational segment reflects a strategic decision on the part of the observer. Time spent with one group rather than another in an organization shapes the fieldworker's perceived character and perspective on events in particular ways. For example, Van Maanen's apparent identification with lower-level officers meant that dissatisfaction and resentment at the way discipline was meted out in the department was more likely to be exposed than would be the case were police managers taken as the source of identification (Van Maanen, 1974:137). In the mediation setting, identification with the mediators rather than their counterparts on the various bargaining committees revealed perhaps more stability and contentment with the process than might have been the case were she to have spent much of her time with the parties to the dispute. A fieldworker, by

becoming closely attached to one group and hence appearing to accept that group's situational definitions will usually cut off access to potential informants in certain other groups. Moreover, in aligning oneself with a given segment over others, observers must also prove their allegiance to the group studied -- as ally, not spy. Such proof will not depend on talk but upon the concrete actions taken by the fieldworker. For Van Maanen, part of his allegiance testing involved his attempts to distance himself from management. For Kolb, allegiance was partly demonstrated through her quiet support of mediators when they were themselves the subject of some dispute.

The police recruits (and patrolmen in general) were particularly sensitive to the possible connections a researcher might have with their bosses. I consciously avoided establishing obvious links with the academy staff and, when asked, turned down offers to sit with staff members at lunch, visit their offices on breaks, or go drinking with them after work. Before entering the patrol division, I met with several union representatives, ostensibly to solicit their advice on how to carry out my work in patrol, but, more practically, to try to assure union officials that I was not a management spy. I also emphasized the partial fact that I had only the most nebulous connections with the ranking officials in the department and, if I identified with anyone in the department, it was with the "overworked but underpaid" patrolmen. My disclaimers seemed to be believed.

Van Maanen, 1981a: 199)

While Van Maanen displayed his allegiance by rather actively ramming it down people's throats, Kolb displayed her's in a far more demur fashion. This reflects more than personal choice or style because both approaches can be seen as more or less alive to the situational proprieties governing interaction in each setting. Patrolmen, for example, regard neutrality, lack of expressed opinion, and any reluctance to get involved in matters of their immediate concern to be features of a morally suspect character. Mediators, on the other hand, take such features as honorable occupational attributes. Thus, Kolb's allegiance was signalled more by what she did not do than by what she did.

My allegiance was to the mediators. When the parties tried to involve me in any way I demurred. Often a spokesman, not necessarily out to undermine the mediator but as a matter

of interest and perhaps additional source of support, tried to solicit opinions from me about positions, the loyal opposition, or the mediator. I was non-committal, claiming ignorance and inexperience. In one case, a management spokesman, as a condition of my participation, requested that at the end of a case I provide an evaluation of the process and the performance of the mediator, I agreed at the time but hoped that in the afterglow of a settlement, the promise would be forgotten. It was. There were other times when spokesmen took me aside to discuss the mediator. Though curious, I resisted these overtures.

(Kolb, 19810: 352)

Although we have used very specific examples to suggest the ways in which we tried to convey impressions about ourselves as committed, reliable and trustable fieldworkers, it would be a fatal mistake to conclude that our behavior on any of these occasions settled such matters once and for all. The impressions wax and wane over time and across people. A field worker may be tolerated for awhile and then, without obvious reason or warning, tossed out of the organization. Minor breaches in the largely implicit research compacts built by the fieldworker and his informants are common enough such that fieldwork often appears to go in waves of carefully building up confidence, then asking sharp probing questions (directly or indirectly) that cause rejection, and then again building up confidence. An often painful tension is created in fieldwork for, in attempting to understand the other, one wants also to be liked. To be liked means playing by the other's rules and, among other things, not asking too many questions. There are no instances in the field, mundane or dramatic, where a fieldworker's commitment, reliability and trust are not of crucial import. In essence, a fieldworker is always being asked, "whose side are you on?" (Becker, 1966). Consider the following appearances of distrust in fieldwork. While seemingly uncommon, it must be said that these illustrations are culled from what, for each of us, is a much larger set.

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Following a family beef call in what was tagged the Little Africa section of town, I got into what I regarded as a mild but entertaining debate with the officer I was working with that evening on the merits of residential desegregation. My more or less liberal leaning on the matter were bothersome to this officer

who later reported my disturbing thoughts to his friends in the squad. Before long I was an anathema to this friendship clique and labelled by them undesirable. Members of this group refused to work with me again.

(Van Maanen, 1981c: 14)

After a settlement had been reached in a particularly complex case, the parties found that they were still unclear about the agreement. The mediator's notes on the case, bound by his convictions of confidentiality, could not be used to clarify the situation. The parties, not the mediator, agreed they would procure my notes and use them as proof for one position or the other. I knew nothing of this at the time. Apparently, attorneys for the respective parties attempted to contact me while I was on vacation. On my next case with the agency, I learned of these efforts. I immediately assured the mediator involved in the case that I would destroy my notes rather than turn them over, demonstrating, presumably, I too respected the confidentiality of the proceedings. While the mediator appeared to accept my assurances, he never again took me with him on a case. Although he never directly alluded to this incident, he also never seemed to have a case starting or going when I called him or met him during my research rounds.

(Kolb, 1981b: 353)

Working Roles

As implied in our discussion thus far, we entered our respective settings as avowed researchers with no plan to hide our intentions or fieldworker identities. Yet, the uses to which others can put a fieldworker are many. Consider an option Van Maanen was provided by certain police officials at the outset of his study.

Before training began, several staff members including the Captain in charge of the Training Division suggested to me that I "go under." That is, these officers presumably felt I would gather better data were my purposes and identity concealed. I resisted their efforts on the grounds that if my true identity were to be discovered during or after the academy, as I felt sure would occur, no one would speak with me again and my research efforts would have to be terminated. This most pragmatic of appeals succeeded. I resisted this dangerous tactic also on the unarticulated grounds that I did not wish to be so enormously dependent upon ranking officials in the department who could "turn me around and out" at will.

(Van Maanen, 1981a: 198)

A covert research role was a virtually impossibility for Kolb given the research bargain which required her to negotiate case access directly with the mediators. Moreover, vis-a-vis the parties, the mediator as the solitary third-party to a dispute is without counterpart. It should be said however that even the overt, forthcoming fieldwork stance has its nuances. Although our research roles were known to most people with whom we had more than fleeting contact, it was the case that the so-called clients of the police and mediators were often misinformed or misled by design and accident as to who we were and what we were up to in a given situation. Such matters may prove troublesome to fieldworkers torn between the desire to be honest about their mission and the equally compelling wish to not jeopardize or contradict the role nicely provided them by the members of the studied group.

On the street, private citizens often times assumed that because I was not in uniform (or, as the police would say, "out of the bag"), I must naturally be a detective or superior officer and such citizens would then direct their remarks to me. My colleagues under such circumstances would usually inform the misled citizen that they were in charge and if there was anything to be said, it should be said to them. At no time did they or I ever make known to a citizen my research status within the organization. There were, however, occasions when my colleagues did not intervene when citizens approached me, either preferring to observe how I would handle the situation myself, or, for various reasons, were busy elsewhere...Short of wearing a clapboard sign, there is no way for a fieldworker to be sure that his research role is the role to which others are responding.

(Van Maanen, 1978:345-6)

Kolb's dealings with the parties to mediation were often of relatively long duration. Conflicting descriptions of her status were common as the following example suggests.

Whether my access was cleared prior to a case (the practice of the federal mediators) or once it had begun (the practice of the state mediators), the mediators nonetheless needed to construct a plausible explanation for why I was in tow. These introductions often distorted my purpose and to stay within the mediator's distortions proved discomforting at times. All the mediators seemed to fill-in an interest in

becoming a mediator on my part although I had not expressed such an occupational concern... The state mediators in particular tended to introduce me as a budding mediator and trainee with their agency. I let this explanation pass on most occasions only to find myself backtracking later. During a case, I often had occasion to chat with spokesmen while committees caucused. As a matter of courtesy or curiosity, they would inquire about my background, training, and aspirations. Concurrently writing a dissertation and training to be a mediator seemed an implausible combination to them and I found myself weaving a new story about my career: "I want to be a mediator, of course, but right now I'm just studying mediation." Such an admission never barred me from continued participation on a case, although several of the state mediators predicted that it might. Given my peripheral status in the scheme of the mediations, it is possible that only I recalled the introduction anyway.

(Kolb, 1981b : 351)

If distinctions between hidden and open status are impossible to consistently manage, the line between participation and observation is even more difficult. We observed more than we participated but from rather different vantage points. Nor was there much question of trying to balance these two orientations for circumstance pulled us toward one end of the continuum or the other. With many of those with whom we interacted, we were merely observer-companions. With others, we were assistants of sorts, doing some of the more trivial tasks associated with the occupation. And, after having completed a good deal of our fieldwork, we took on some rather special duties in the company of a few. Consider, first, Van Maanen's description of his working roles in the patrol division.

To some officers with whom I worked, I was a sort of acceptable incompetent, able perhaps to shorten the long hours on patrol through conversation, but unable to do anything remotely connected to the job itself. To most officers, I was the reserve officer, a well-knowntype in the organization. In this role, I was a friendly helper who could, when called upon, handle some light paper work, be responsible for radio, conduct an interview at, say, the scene of a fender-bender traffic accident; but, nonetheless, required continual supervision and could not be assumed to know what to do should an occasion arise in the field that called for "real police work." To a very few officers, two or three at most, I was more or less a working partner, albeit a temporary one.

As an acceptable incompetent, I sat in the backseat of a two-man unit, taking no part in the decisions being reached in the frontseat, save those about where and when to eat lunch or take a break. On these shifts, I rarely spoke with anyone but my police guides. I did no police work other than to keep a personally protective eye on a prisoner who sometimes shared the backseat with me.

As the friendly helper, my time was split somewhat evenly between one-man and two-man units. In this role, I was delegated tasks such as keeping the log (ie, a running account of the activities engaged in by that unit during the shift) or calling radio for a license plate check on a vehicle that just might turn out to be stolen. Other times, I would be asked to post myself at the corners of a building when investigating a potential burglary or prowler call. As the friendly helper, I was clearly expected to use my body or whatever other means I had available to back-up and assist an officer if any altercation arose during the tour.

Finally, as a working partner, I was put in the role of what Union City policemen called the "shotgun partner." I played this part only with officers working solo beats and during these tours I was responsible for radio communications, paperwork (signing my name to the log, arrest reports, field investigations, etc.), back-up responsibility on traffic stops (positioning myself just outside the passenger door on the patrol car), and so on. On calls such as the various sorts of disturbance calls, I would help separate the quarreling parties, restrain them if need be, and usually share in the decision making surrounding what, if any, actions were to be taken. On no occasion, however, did I drive a prowl car on routine patrol for probably the same reasons few rookies drive -- veteran officers do not trust the novice driver who, one, does not know the district, and, two, is unaccustomed to the unpredictable way other motorists react to the "gumball machine."

(Van Maanen, 1981a: 202-3)

Kolb's working roles were of the following sorts.

I spent most of my time as a student-companion, by the mediator's side, avidly soaking up his perspective, wisdom, and commentary. I listened to his renditions of the case, the agency, the state of mediation generally, gossip of other mediators, vacation plans, versions of how the local ball teams were doing, how his children's little league teams were managed, and, on one case, picked some horses. Many mediators remarked how pleasant it was to have me for a companion. At times, mediation can be very lonely. With no colleagues about, excluded from committee caucuses, a mediator spends much time by himself in his office or roaming the halls in strange public buildings. Any kind of companionship is welcomed. But, apart from seeming to lighten the load of social isolation, I was, in this role, totally dispensable to the proceedings. Just how dispensable became clear when I was sent home from two cases because the mediators had apparently judged it unsafe for me to be out so late.

As time wore on, I volunteered for certain amenity-providing tasks and, as a result, became, for some mediators and on some occasions for others on the scene as well, something like a good secretary. I got coffee and other refreshments for the parties and the mediator. I xeroxed copies of proposals or copied them from the board at the mediator's request. Mediators would occasionally have me consult my notes on the case when a question arose and their own memories failed or notes were incomplete. It seems, however, the major difference between the student-companion role and the good secretary was the degree the mediators looked upon me for support. Thus, as a good secretary, I became the person to whom they could recite their tales of woe, a non-critical ear for their version of the untoward event. There were several occasions when a mediator said or did something which brought forth condemnation from the parties. The mediators so accused would, at their first opportunity, take me aside to explain what really happened. These were situations where the mediator felt he had been used or intentionally misled. I was the only one available to listen to such betrayal tales. There were also, as one might expect, lengthy asides given to me by mediators designed to insure that my reading of the "unproblematic" aspects of the case was the correct one (ie, the same as theirs). It may of course be the case that my presence on the scene as an audience to what the mediator might regard as a mistake inevitably called forth these account-giving sessions. For whatever reasons, however, I provided the sympathetic ear into which the mediators could speak their unopposed view, a common secretarial function.

Perhaps because mediation is individually-based, mediators express considerable curiosity regarding current practices and trends in settlements as well as what kinds of performances their peers are putting on in other disputes. Thus, for some mediators, I became a source of information about inter- and intraagency practice. Moreover, based, I assume, on my willingness and ability to provide useful information, two mediators came to see me as a resource, a consultant of sorts. These mediators solicited my advice on how I would handle certain matters. For example, one of these mediators was once berated by a spokesman for failing to understand the demands of his committee and, at the same time, berated by the other spokesman for exerting undue pressure on his committee. These accusations were couched in strong language and when, in private, the mediator asked me for my assistance, I first lapsed into the part of the good secretary and merely listened. But, it soon became clear that he wanted more than a sympathetic ear and I then suggested some actions he might consider trying. More generally, my knowledge was also sought out at times on issues of labor law and public policy. I suspect because of my participation on a variety of cases as well as my special student status, some mediators looked to me for expertise. On occasion, they seemed to assume I was far more informed than, in reality, I was. But, even when I thought I knew an answer, I usually replied with circumspect care to such queries. In one case, an attorney argued that fringe benefits were not exempted from the wage-price guidelines. The mediator

disagreed and turned to me, his expert, for support. To my undoing, I supported the attorney's position. After the session, the mediator chided me in private for "queering his pitch." He claimed to know full well that the attorney was right but had hoped to intimidate the committee with his action. I became, as a result, far more reticent about contributing my meager advice and knowledge.

(Kolb, 1991b: 354-62)

What comes through as a result of this brief comparative look at our fieldwork roles is the variability associated with them. At times, Van Maanen frisked suspects, put handcuffs on them, wrote assault reports, while, at other times, he simply stood in the shadows watching the police go about their tasks. Similarly, Kolb convened meetings, delivered messages to the parties, and aided in a variety of subterfuges mediators occasionally used to extract spokesmen from the den of their committees. At other times, she remained in the hall while the mediator caucused privately with the spokesmen in his office, maintained a servile stance as mediators displayed their knowledge to the various parties in a dispute, or acted as the proverbial go-fer, fetching coffee and various other tools of the mediator's trade.

There are several points worth making about these consistent inconsistencies. To participate in the activities of the studied and indeed to observe and question people on the scene in contextually sensitive ways, fieldworkers must cast themselves in roles that are culturally meaningful to the studied. In the absence of such roles, members will experience considerable difficulty in establishing relationships with the fieldworker that go beyond the most prefuntatory sort. A vignette from Kolb's study makes this point.

I came to consider myself a learner of the process and found that acting as a trainee enabled mediators to adopt a seemingly natural posture towards me. This decision emerged as a result of a fortuitous but jarring experience. One day early in the study, before I had yet an occasion to observe a case, I had a chance encounter with three FMCS trainees who were assigned to the field office as apprentices to the local mediators. We spent several hours discussing their training and the experience they had had with the mediators I would eventually be observing. I learned that the "side bar" was the major vehicle through which trainees felt they came to understand the practice of mediation. A "side bar" was jargon for

private meetings between the mediator and trainee where on-going strategies were reviewed as a case progressed. I too wanted mediators to discuss their strategies with me but found that I was having great difficulty in my interview-like conversations to date. With one mediator in particular, I was having problems. He was quite reluctant to answer my questions about mediation. He did however recount an experience he had with other researchers. Apparently, the FMCS "brass" had sent two academics writing a book on mediation to see him. They questioned him about his theory of group dynamics and how he used peer pressure in dispute resolution. To him, it was not at all clear what these well-meaning researchers meant. In light of his story and what I had picked up from the trainees, I began to change my interviewing strategy. Thus, when I asked this mediator to give me a reading on a case as if I were Jim Jackson (a trainee), he talked for twenty minutes. Until that point, I had not realized that without putting myself into a familiar role, mediators were unclear about what I wanted from them.

(Kolb, 1991b: 355-6)

It is quite obvious that a culturally meaningful role in one setting may not work in another. In the police world, where patrolmen rarely tell one another anymore than they have too, the trainee or recruit role only allows access to other trainees or recruits. Van Maanen's inchoate strategy was to remain with these recruits as a colleague until they became -- and, by association, he too -- full-fledged police officers with at least a modicum of street experience behind them. His research relationships underwent predictable shifts.

In Union City, I developed friendships that have gone far beyond the study itself. In the process, I have attended parties, spent many evenings talking, drinking, playing cards, shooting pool, and, in general, socializing with the police. There is no doubt in my mind that participating in the social life of policemen is directly related to the degree to which the researcher will be allowed to participate in the work life of the police (and vice-versa). To draw a rigid distinction between the two spheres of life would be to do something the police could not, nor would not, do.

(Van Maanen, 1981a: 201)

Exit and Betrayal

The cultivation of relationships with those studied is, as we have indicated, a prominent and on-going part of ethnographic fieldwork. Yet, those relationships are temporally bound although it is often convenient for those involved in them

to overlook this fact. At its core, fieldwork must be regarded as something of a traitorous activity. Since discretion is usually part of the price one pays for insider status, the fieldworker automatically violates, if not mocks such discretion when reporting on the life of the observed. Despite whatever care is exercised by the fieldworker in protecting personal and institutional confidences, the people whose activities are reported upon are quite likely to wonder at the researcher's point of exit (and well beyond) whether or not they have been fooled, exploited, misunderstood, or otherwise made patsies of by the researcher. Hughes (1974:33), with characteristic clarity, put this dilemma well: "The fieldworker is inevitably a spy, a double agent, who will betray his subjects ... but with the hope that in the end the truth will help us all out." In this light, consider how some of those involved in our studies have reacted to our public versions of their work.

A few officers in Union City were angered by my writings. However, they were not so much upset apparently at any untruths or distortions in my writing, but rather they expressed discontent over the matters I had chosen to report upon. And, not unexpectedly, they were far angrier at the person who would chose to write about such matters than they were at the matters themselves. Apparently, it was my character, motives, and morality that were to be called into question and not the accuracy of the reporting.

Van Maanen, 1981a: 225)

The reaction of some mediators to her writings was suprising to Kolb.

Although I attempted to keep my assessment of the relative merits of orchestrating and dealmaking relatively benign, the bias in favor of the federal mediators does, in my mind, come through. Yet, the state mediators enthusiastically vouched for the dealmaking version of their practice I have presented. Orchestrating, however, was read by some federal mediators as passivity, a stance that runs counter to the public presentation mediators put forth for their work. Other mediators who have read my materials critically certain elements of it by dismissing my findings as atypical, the result of stupidity and incompetence among the studied mediators and, therefore, a partial description. It has also been suggested to me that I may do irreparable harm to mediators by portraying them as anything less than forthright, neutral, and effective.

(Kolb, 1982, personal correspondence)

Despite one's intellectual acceptance of the inevitability, indeed, the welcome addition of disagreement and alternative frameworks, the sometimes nasty and unpredictable reactions of the observed is altogether ~~emotionally~~ *emotionally* bothersome to a fieldworker. Since member tests are one of the more stringent criteria useful for evaluating the validity of one's work, such reactions contribute to the lingering doubts as the veracity of the tale that was told. Even if the fieldworker subscribes to the "wheres-there's-smoke-there's-fire" school of ethnographic verification (Douglas, 1976:66) and takes such distaste as an indication that some crucial matters of the group under study have been revealed (matters group members would be far happier keeping under wraps), critical reactions are still troubling. Part of this matter lies we think with the fact that research relationships themselves are irrevocably altered upon publication of fieldwork results.

This discontent evoked by second-guessing one's own analysis prompted by critiques is not merely a wistful pang for what has passed; but, serves as commentary on what is to be as well. For us, the retreat from the field and the eventual publication of research reports has marked not the end of study, but the beginning of what appears to be a lifetime pursuit of the increasingly subtle implications that lie in our work -- including the not-so-subtle one regarding whether or not we got it right the first time. It is also the case that one distinguishing characteristic of ethnographic research is the close relationship established between the student and the studied. And, like all close relationships, one is never quite sure they have fully understood the other. Each brush with the data alerts one to the essential incompleteness of all descriptions and understandings. There is always so much more to say. Van Maanen has been back to the field a number of times since first leaving Union City in 1970.¹⁴ He has also had numerous occasions to work with the police in other contexts.

Over the past ten years, I've taught many classes in which the students have been almost all police officers. In virtually every class, I find myself rushing through my formal classroom materials so I can more or less 'get down' and engage myself with the students in long, rambling, high spirited discussions of the police world. The tales are seemingly endless though we all seem to realize (tacitly) that we are all liars. I have found that one can learn as much from the well-told lie as from the well-told truth.

(Van Maanen, 1981, personal correspondence)

Kolb is currently planning another mediation related study and has recently been involved in a training program for mediators. On the latter, her remarks are indicative of a fieldwork junkie.

The training sessions revolved around current experiences the mediators were having and discussions centered on the kinds of strategies mediators made use of in resolving problematic situations. The agency director reported to me that a few mediators were concerned about my notetaking during the session I was ostensibly running. I assured the director that my notes were only a means of keeping track of what was going on and I used them to jar my thinking between sessions. In point of fact, I thought I was seeing previously unseen dimensions of mediation and I wanted to be sure I would recall them later for my own purposes, not theirs.

(Kolb, 1982, personal correspondence)

Since our career paths do not include policing or mediation as full-time occupations, we seem somehow to be self-cast in the roles of professional apprentices who will never quite make it to journeymen. This is irony but it is also fieldwork.

The Professional Apprentice as Fieldworker

Our work as described in this paper follows no obvious prescribed or formal form. It is as if the box we have tried to stuff ethnography in belongs to Pandora. Whatever order we have made of it here is both retrospective and arbitrary. Yet, there does seem to be some conceptual commonality associated with our work which will allow us to at least shut the lid on our box even if we have yet to neatly arrange much of its content .

One commonality is that our work is predominately qualitative in the sense that we seldom made counts in the field and typically have expressed our findings in natural language. On the surface, the differences between quantitative and qualitative research can be seen in the reliance on differing notational schemes -- numerical versus narrative. Such surface contrasts reflect much deeper differences in the aims, commitments, and everyday research protocols for doing organizational studies.

Fieldwork presents a problem to the researcher primarily because there is virtually no clear evidence available before a study is undertaken as to what the data should be and how such data can be generated. It is obviously a gloss to suggest merely that the fieldworker get close to people, win their trust, and return from the field with a description of the life that was found there. While ethnographic researchers seek access to particular locales or social scenes, they also seek access to the multivalent meanings and other inner phenomena various people in these settings utilize when going about their normal affairs. The former task is, in comparison to the latter, far less problematic. Although one may not know precisely where they are, they will at least know they have arrived somewhere.

One strategy for finding out where one has arrived is simply to ask those on the scene. In this sense, the experts of ethnographic studies are the residents, natives, informants, members, or so-called lay actors in the studied scene. Instead of trying to find out something about the organization that people within it do not know, fieldworkers attempt to find out what it is they do know. This conflicts perhaps with certain well known aims of social research, for example, discovery and progress; but, these aims are replaced by what we regard as equally important ones, description and understanding. What lay actors see and acknowledge as important (and unimportant) aspects of their life-worlds are findings in ethnographic inquiry. The view here

is Weberian: Social science must deal with social acts which, by definition, are suffused with subjective meaning(s).

A persistent problem here is that such subjectivity hardly leaps out of a crowd of observations to present itself to the fieldworker. An interpretive task faces the ethnographer and it is a highly reflexive one for the raw data themselves may represent interpretations already arrived at by those on the scene (or, interpretations of interpretations, etc.). Everyday life, as the special province of ethnographic research, is, for both the observer and the observed, a preanalyzed domain. Only in the role of stranger does conceptual complacency waver. But, it is just this stranger role the fieldworker tries quickly to shed. Moreover, as newcomers know so well, a huge amount of uncertainty reducing structure can rapidly be imposed on unfamiliar surroundings (Van Maanen, 1977:15-48; Louis, 1980). Such structure may prove inadequate of course but it will still be imposed.

Our method for circumventing (or, perhaps, coping) with the persistent problems of perspective, understanding, and interpretation has been to adopt the apprentice role in the field. When we were viewed as a "wannabe" police officer or mediator, we felt our informants were willing to show us how they accomplished their various tasks rather than to simply tell us. As novices, the information available to us seemed largely determined by the particular activities at hand. What informants selected to describe and to ignore were materials we could assess when we tried to apply what we learned to on-going tasks. Instructions, then, were the distinctive sort of data we gathered and, as such, there were some most practical tests available as to our good informants and bad. Moreover, the crucial verification of our work in the field becomes lodged in our own learned ability to perform the tasks studied. It is a member test but a member test with a phenomenological twist.

The fieldworker's understanding of the social world under investigation must always be distinguished from the informant's understanding of this same

world. Although we believe we have learned to think like cops or mediators, we are still presumptuous enough to also believe we can both describe and critique these frames of reference. Such a state of mind is intellectually curious and is, most definitely, not characteristic of most of the people we studied who take-for-granted much of what we regard as highly relevant and unexpected fact. There is a very real difference in the kind of knowing that occurs when one is responsible for one's actions as a member in good standing of a particular organization and the kind of knowing that accompanies the limited responsibilities assumed by the apprentice. To argue that we have become part of the worlds we studied or even that we understand them in precisely the same way those who must live within them do would be in grave error.

For us, then, the fieldworker is a professional apprentice. But, we use this descriptive phrase with some caution. ~~We use it~~ as a slogan to remind ourselves that much of what goes on in the fields of our study, we do not yet know (nor, probably, ever will). The governing term of the slogan is "professional" because, however far we move toward the "apprentice," we always know we will leave the field. In this sense, fieldworkers can never fully apprehend the world of their informants in its "natural" form. Involvement and identification are at best only transitory since fieldworkers are not solely interested in what things are for themselves as are the people they study (Bittner, 1973:113-4). Rather, the fieldworker is interested in what these things "stand for" to the group observed. The difference is a matter of profession and it will not (nor should not) go away.

NOTES

1. This paper drastically abbreviates an uncountable number of lengthy conversations between the authors, discussions held before and after one or both of us had been to the field. At various time, in various places, far too many people to mention here have knowingly and perhaps unknowingly provided wise counsel. We have experimented with several formats for this paper from the sort of systematic and analytic comparison of our methods set up along formal dimensions of contrast to a single-voice, impersonal cookbook approach. The first was unsatisfying because our dimensions kept dissolving under scrutiny; the second unsatisfying because we kept finding uncomfortable violations we ourselves committed of our own normative code (even when we tried to invent the code to, colloquially but accurately, cover-our-ass). The version presented here represents a fall-back position; a narrative, spiced up from time to time with a hesitant generalization or two and held tentatively in place by the ritual introduction, footnotes, and conclusion. The narrative itself is an odd sort: Two fieldworkers (set off in time, space, and typography) commenting by virtue of selected examples on equally selective portions of their work. This is then pulled together by a kind of cool voice-over whose doubled authority keeps things more or less moving. Learned students of the text could have a holiday with this paper. We had fun too. Partial support for the writing was provided by: Chief of Naval Research, Psychological Sciences Division (code 452), Organizational Effectiveness Research Programs, Office of Naval Research, Arlington, Virginia, 22217; under Contract Number N00014-80-C-0905; NR 170-911.
2. While we define ethnography in terms of an attitude, other, more popular, definitions exist. One of our favorites is Sanday's (1979:527) use of an image: ethnography is the fieldworker stepping into strange surroundings prepared for a long stay. Conklin (1968:172) provides the standard procedural definition: "Ethnography involves a long period of intimate study and residence in a well-defined community employing a wide range of observational techniques including prolonged face-to-face contact with the members of local groups, direct participation in some of the group's activities, and a greater emphasis on the intensive work with informants than on the use of documentary or survey data."
3. Good examples are apparently cherished by those who come into contact with them but why this is so is somewhat puzzling. What it is that makes an example good is no simple matter. Spicy, counterintuitive, trenchant, clear, cryptic, bounded, and so forth are all one word answers but suggest no analytic insight. Examples also serve as what Schwartz and Jacobs (1979:293-4) call a "promissory note" in that if they are found once, presumably they could be found again. In our mind, good examples are all of these things plus they are context sensitive, suggesting to the hearer or reader of an example just what interpretive framework is being laid over the material. What often distinguishes ethnography from case study is just this latter point. See, also, Davis (1974).
4. See, for example, Bosk (1979), Manning (1977), Willis (1977), and Wolcott (1973) for ^{recent} exemplary (ethnographically-informed) analyses of hospitals, police agencies, working class careers, and schools, respectively. Splendid articles in this genre include: Roy (1960), Clark (1960), Strauss (1964), and the collection organized by Berger, ed. (1964), and Bail (1967).

Some more current articles include: Slosar (1973), Haas(1972) and much of what is found in the journal Urban Life. Recent quasi-ethnographic work(extended, contextual interviewing) in mainstream corporate environments is represented by Kanter's(1977) influential book. Classic references include Dalton(1959), Gouldner(1954), Goffman (1961), and Becker et al.(1961), Hughes(1931) is still a good read.

5. Psychologists have been particularly attentive to this phenomenon under the label "demand" or "experimenter" effects (e.g., Rosenthal, 1966; Orne, 1962). The Heisenberg principle in physics is somewhat different than its carry over in social science. As we understand its use in physics, it refers to the notion that knowing precisely one thing about an object makes it impossible to know precisely another thing. If we know something's speed, we cannot know its position (and vice-versa). This is a measurement artifact and it will not go away. In social science, the Heisenber principle refers to the frequent fact that in the process of observing human behavior we change the very behavior we seek to observe. Like everything else in social science, the principle is a variable.
6. Douglas(1976) regards penetrating fronts as the essence of fieldwork in one's own culture. Berreman (1962) says the same of other culutures. It would seem that we have moved some distance from the initial formulation of ethnography in both social anthropology and Chicago School sociology wherein trust stood above distrust as the proper attitude of the fieldworker at work. Cynicism and perhaps the fear of being put-on seems to be the stance taken by the "new ethnographers". See Van Maanen(1981b) for some "front busting" plays.
7. By "ritual constraint" we mean those expressive implications of a fieldworker's presence and activity in a research setting. Following Goffman(1976:282), everything the fieldworker does (or does not do) in the field can be read as portraying the position he takes up regarding matters of consequence to the observed. Much of what is done is rather blissfully ignored of course but when it is not, we may have a ritual constraint in hand. What the fieldworker says represents but one class of these constraints. Other classes are pointed to in following sections.
8. On the anthropological side, see, Radcliffe-Brown, 1958; Malinowski, 1961 (1-25), 1967; Frake, 1964; Powdermaker, 1967; Freilich(ed), 1970; Pelto, 1970; Kimball and Watson, 1972; and Spradley, 1979. On the sociological side, see Junker, 1960; Bruyn, 1966; Habenstein(ed.), 1970; Filstead(ed.), 1970; Lofland, 1971, 1976; Johnson, 1975; Suttles, 1976; and Schwartz and Jacobs, 1979. The sociological primer for all fieldwork is, of course, Goffman(1959). One of the best mergers of the two disciplines is Agar(1980) from whom we borrowed many ideas including the one for the title. This literature is growing fast and students of organizations may find it difficult to keep up(we certainly do). Some help here may be Weick's(forthcoming) rather exhaustive yet characteristically pithy and idiosyncratic review of the "unobtrusive methods" literature. In Weick's view, because fieldwork is so obtrusive, it becomes unobtrusive for those caught in its glare. See, also, the special issue of Administrative Science Quarterly (24,4,1979) on Qualitative Methods.
9. FMCS employs 300 full time mediators who work out of the national office in Washington, D.C. as well as seven regional offices and 72 field offices across the country. Established by the Taft-Hartley Ammendments to the National Labor Relations Act(NLRA), FMCS has jurisdiction over disputes in the private sector, non-profit health facilities(NLRA ammended 1974), and workers employed by the federal government(Executive Order 11491, 1970). Twenty-eight state agencies oversee mediation activity in their respective jurisdictions. The actual number of mediators engaged in this activity is difficult to gauge because state laws differ regarding the jurisdiction of these agencies and whether full-time or ad hoc mediators

are used to work disputes.

10. There are occasions when more than one mediator may work on a case. Certain state laws call for tripartite mediation boards with members selected from the ranks of management, labor, and neutrals. The law in Massachusetts does not prescribe this structure. Less formally, a mediator may request the services of a colleague in a case where the original mediator feels his ability to work with the parties has been compromised or where, as a matter of tradition, "top brass" from the region or headquarters "rides in" to resolve the dispute.
11. In the text this remark is drawn from, Van Maanen has a footnote worth footnoting again. "I must rank among my faux pas in Union City this agreement to keep confidential the name of the department. I was not asked directly about this matter, but volunteered such an agreement in my initial meeting with the Chief of Police. I did so out of perhaps a sense of nervous anxiety and, correspondingly, an overwhelming desire to please my potential departmental sponsors. However, once my promise was out, there was no backing away from it for I was reminded of the agreement many times before I departed. Whether or not departmental anonymity would have eventually surfaced as a condition for continuing the research, I can not say, but, nonetheless, the fact remains that I did blunder into what I now consider an unfortunate artifact of too many police studies, an artifact that limits our cross-departmental understanding of police systems." (Van Maanen, 1979: 227)
12. The terms "primary and secondary access" are drawn from Manning (1972). They are meant to draw attention to the recursive properties of negotiating entry for fieldwork in organizational settings. Primary access is something like a fishing license, secondary access represents the catch. It should be noted that secondary access is an on-going concern in fieldwork since it unfolds at the individual and small group level.
13. A savage yet oftenheard remark is that 'fieldworkers rarely tell us the worst thing that happened to them in the field, only the second (or third or fourth) worst thing.' With this observation, we agree, although we will also note in passing that this self-preservation domain is hardly occupied by fieldworkers alone. Imagine a survey researcher prefacing his methodological remarks by noting: 'Subjects were crammed side-by-side into a large auditorium late on a Friday afternoon of a record hot August day and asked to complete a 22 page questionnaire amid coughings, stomach rumblings, and other respondent noises while those administering the questionnaire packet milled about the breezeway chatting pleasantly with one another.' At any rate, we have too few examples in this domain.
14. On some of the special properties these return visits to the field promise the fieldworker, see Van Maanen (forthcoming).

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